Housing Vouchers, Neighborhood Effects and Economic Self-Sufficiency: Evidence from a Randomized Experiment

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Abstract

In this paper we examine neighborhood effects on labor market outcomes and welfare receipt using data generated by a randomized housing-mobility experiment. Our outcome measures come from state administrative records for quarterly earnings and employment, and participation in public assistance programs. We find that the opportunity to relocate to very low-poverty neighborhoods reduces the probability of welfare receipt but has no measurable effects on earnings or employment as measured by quarterly unemployment insurance records.

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expressed here reflect those of the authors alone.

I. INTRODUCTION

From 1970 to 1990, the number of people in the United States living in high-poverty census tracts[1] nearly doubled, from 4.1 to 8.0 million (Jargowsky, 1997). The persistence of inner-city poverty in the face of government anti-poverty policies and even extended periods of economic growth has led many observers to conclude that residence within a high-poverty inner city neighborhood contributes to the economic difficulties of residents (Teitz and Chapple, 1998). Some economists believe that the social composition of one's neighborhood affects employment probabilities through access to job referrals and information about job opportunities (Wilson, 1987, 1996, Montgomery, 1991, Holzer, 1996, Topa, 1996, Mills and Lubuele, 1997). The behavior and attitudes of one's neighbors may also influence the social support provided to those who choose to work or the stigma attached to decisions to go on welfare (Wilson, 1987, 1996, Moffitt, 1983). The "spatial mismatch" hypothesis emphasizes the geographic distance between inner city neighborhoods and suburban job opportunities – minorities who are unable to move to the suburbs because of racial discrimination experience low net returns to working because of high commute costs, or depressed wages due to crowding in less-skilled urban labor markets (Kain, 1968, 1992, Holzer, 1991). Further, the ratio of "marriageable men" to women in economically distressed neighborhoods may be low, which in turn impedes the formation of economically viable two-adult households (Wilson, 1987).

Motivated in part by the belief that neighborhoods affect economic self-sufficiency, the fiscal year 1999 and 2000 budgets for the U.S. Department of Housing and Urban Development (HUD) included funds for 75,000 new Section 8 housing vouchers and certificates (hereafter "housing vouchers"[2]) as part of the Clinton Administration's Welfare-to-Work initiative (HUD, 1999). In fact, in 1998, more families received housing vouchers from HUD (1.4 million) than lived in public housing buildings (1.3 million).[3] By providing families with a subsidy that enables them to move into private-market apartments in other parts of the city or suburbs, vouchers in principle may improve the quality of housing services that low-income families consume, reduce the concentration of poverty in our central cities, and expand economic opportunities.

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[1] Jargowsky defines high-poverty census tracts as those with poverty rates of at least 40 percent.

[2] Both Section 8 rental certificates and vouchers are issued to income-qualified households, and "pay [private-market] landlords the difference between the tenant portion of the rent (30 percent of adjusted income, 10 percent of gross income or the portion of welfare assistance designated for housing) and the contract rent, which must not exceed the HUD-established fair market rent for the area" (<<www.hud.gov>>). For our purposes, we treat the two tenant-based programs as equivalent and term them "housing vouchers."

[3] From the U.S. Department of Housing and Urban Development, <<www.hud.gov/cfda>>, downloaded July 24, 1999.

The effects of housing vouchers on economic self-sufficiency will depend on the kinds of neighborhoods into which families move, and the effects of neighborhoods on earnings, welfare receipt and family formation. On the first point, the Experimental Housing Allowance Program (EHAP) of the 1970's suggested that housing subsidies did not substantially increase residential mobility, or change the characteristics of the neighborhoods into which families moved (Struyk and Bendick, 1981). Yet providing public housing residents with vouchers may induce more mobility than what was observed with EHAP because the baseline conditions in public housing developments are likely to be far inferior to the average neighborhood of EHAP participants. Moreover, racial discrimination was presumably less of a barrier to economic and racial integration in the 1990's than the 1970's.

Even less is currently known about the effects of neighborhood characteristics on economic self-sufficiency because of a basic identification problem: Since almost all families have at least some degree of choice over where they live, variation in economic outcomes across neighborhoods may reflect either the causal effects of neighborhood conditions or the effects of unmeasured variables that are correlated with both residential decisions and economic success. The magnitude and even direction of bias that may result is difficult to determine. While most analysts assume that more motivated families who are likely to be successful in the labor market will move to lower-poverty neighborhoods, in principle it is also possible that families who are most confident in their ability to navigate life in distressed neighborhoods (and the challenges of the labor market) are the ones who take advantage of the lower housing prices in these areas. Because non-experimental estimates may be subject to either upward or downward bias,[4] interpretation of the quite mixed literature on neighborhood characteristics and labor market outcomes is difficult.[5] The most convincing evidence available to date comes from the quasi-experimental Gautreaux program in Chicago, which suggests that families who are moved to the suburbs had higher employment rates but similar wages compared to city movers (Rosenbaum and Popkin, 1992, Rosenbaum, 1995).

The present paper examines the effects of neighborhoods on economic success, and the potential for housing voucher programs to improve economic self-sufficiency, using data generated by a randomized housing-mobility experiment. Since 1994, the U.S. Department of Housing and Urban Development's Moving to Opportunity (MTO) demonstration has assigned a total of 638 families from high-poverty Baltimore neighborhoods into one of three different "treatment groups": *Experimental group* families receive housing subsidies, counseling and search assistance to move to private-market housing in low-poverty census tracts (poverty rates under 10 percent); *Section 8-only group* families

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[4] Note that the same omitted variables bias is possible with studies of teenage employment rates. While teens are unlikely to make family decisions about residential location, unmeasured family variables that affect residential choices may in principle be relevant for adolescent behaviors such as employment.

[5] For reviews of the relevant literatures see Mayer and Jencks (1990), Holzer (1991), Kain (1992), and O'Regan and Quigley (1997, 1998).

receive private-market housing subsidies with no constraints on relocation choices; and a *Control group* receives no special assistance under MTO. The randomized experimental design of MTO thus breaks the link between family residential preferences and adolescent outcomes, and helps us overcome the self-selection problem found with previous studies.

Our outcome data come primarily from state administrative data. We measure quarterly employment and earnings using records maintained as part of Maryland's unemployment insurance (UI) system. Receipt of public assistance (PA) benefits are measured using administrative data from the Maryland Department of Human Services. We also measure changes in household composition using baseline and follow-up survey data collected by Abt Associates. We find that the proportion of householders who receive PA benefits is lower among the experimental and Section 8-only groups than in the control group. At the same time, we find little difference in quarterly earnings and employment rates as measured by state UI records. The most plausible explanation for these findings in our view is that families experience an increase in earnings opportunities that are not captured by UI earnings data. We hasten to add that since MTO families are a self-selected group of public housing residents, our findings may not generalize to other low-income populations.

The paper is organized as follows. The next section describes the MTO experiment in greater detail. The third section discusses the conceptual framework for our analysis. The fourth section discusses the data used in our study. The fifth section presents empirical results for the mobility outcomes of MTO families, quarterly employment and earnings, welfare receipt, and householder cohabitation. The sixth section discusses the implications of our findings for future research.

II. THE MOVING TO OPPORTUNITY DEMONSTRATION

The Moving to Opportunity demonstration is based in five cities: Baltimore, Boston, Chicago, Los Angeles, and New York. The present paper uses data from the Baltimore site, where eligibility was restricted to very low-income families with children who lived in public housing in one of the five poorest census tracts in Baltimore City. The average poverty rate in these tracts in 1990 was 67 percent (Goering, Carnevale and Teodoro, 1996). The baseline neighborhoods are also notable for a paucity of affluent neighbors, which previous research suggests has a distinct effect on youth outcomes from neighborhood poverty (Brooks-Gunn, Duncan and Aber, 1997). Less than five percent of households in these tracts had annual incomes of $50,000 or more (in 1990 dollars), and less than seven percent of adults in these areas had a college degree.

The program was publicized in the baseline tracts by the Housing Authority of Baltimore (HAB) and a local nonprofit, the Community Assistance Network (CAN). Families who volunteered for the program were added to the MTO waiting list. Families were drawn off the MTO waiting list over time on the basis of a random lottery, and then randomized into one of the three MTO treatment groups. Both types of randomization were conducted by Abt Associates.

Families in the experimental and the Section 8-only groups were assigned Section 8 housing vouchers or certificates, which provide subsidies to lease private-market housing. As part of the program's design, the Section 8 subsidies provided to the experimental group can only be redeemed for housing in census tracts with 1990 poverty rates less than 10 percent. Families in both the experimental and Section 8-only groups had up to 180 days from the time at which they begin the housing search to identify a suitable rental unit and sign a lease.

The experimental group also received services from CAN, the local Baltimore nonprofit, including assistance to resolve credit problems and to locate and lease suitable rental housing. Before the housing search was initiated, CAN also required experimental families to complete four workshops on topics such as budgeting, conducting a housing or job search, dealing with landlords, and conflict resolution. Families in the Section 8-only group receive no additional assistance beyond what is provided to all participants in HUD's Section 8 subsidy program. Families in both the experimental and Section 8-only groups were required to sign leases for one year. Those who wished to move again before the initial lease expiration date were not eligible for a new Section 8 subsidy. Families who wished to relocate with their subsidy after the first year were able to do so without restriction. CAN contacted experimental families twice following relocation; otherwise, post-program monitoring was limited. (For additional details on MTO see Goering, Carnevale and Teodoro, 1996).

III. CONCEPTUAL FRAMEWORK

The ways in which neighborhoods may affect family outcomes can be highlighted using the reduced-form equation (1) from Moffitt (1998). Some outcome y_{in} for individual (i) in neighborhood (n) will be a function of the householder's characteristics, x_{in}, such as educational attainment and labor market experience, and the characteristics of those living in family (i)'s neighborhood, $x\text{-}_{in}$.[6] The equation also allows for an effect from neighborhood attributes $,_n$ that are difficult to measure in practice, such as distance from job opportunities (Leonard, 1986, Raphael, 1998), and unobserved factors specific to the family and neighborhood, $,_{in}$.

$$(1) \qquad y_{in} = \$_0 + \$_1 x_{in} + \$_2 x\text{-}_{in} + ,_n + ,_{in}$$

The fundamental challenge in identifying the parameters in equation (1) stems from the possibility that unmeasured individual-level variables that affect residential choices are also correlated with economic outcomes, as in equation (2). In this case, ordinary least squares estimates of (1) with nonexperimental data will confound the causal effects of neighborhoods with the effects of the

[6] In principle we could allow for a separate effect of the value of the outcome among one's neighbors, $y_{\text{-}in}$, but Manski (1993) and Moffitt (1998) argue that it will not be possible to distinguish between the effects of neighbor background characteristics and neighbor's behaviors with respect to the outcome variable of interest.

unobserved family-level variables. MTO helps overcome this problem by randomly assigning families into different mobility treatment groups.

(2) $E [x\text{-}_{in} , _{in}] ... 0$

In practice only the family's treatment group is randomly assigned, not their actual mobility outcome. We overcome this problem by comparing mean economic outcomes of all families assigned to each of the three MTO treatment groups, regardless of the family's relocation status. These "intent to treat" effects are obtained by estimating equation (3), where E_i and S_i represent dummy variables equal to 1 if the family is assigned to the experimental or Section 8-only groups, respectively. The difference in mean outcomes between the experimental and control groups is given by the parameter $"_1$, while the Section 8-only / control comparison is given by $"_2$. These effects are identified because E_i and S_i are orthogonal to the error term in equation (3) by virtue of the MTO program's randomized experimental design. The regression equation also includes a vector of baseline family characteristics x_{in} to adjust for random differences in these variables and improve the precision of our estimates; because of random assignment, inclusion of these variables should have only modest effects on our point estimates for $"_1$ and $"_2$.

(3) $y_{in} = "_0 + "_1 E_{in} + "_2 S_{in} + "_3 x_{in} + <_{in}$

While the MTO experimental data enable us to overcome the problem of self-selection into different neighborhoods, we cannot identify exactly which neighborhood characteristics are responsible for any observed effects. The reason for this identification problem is because MTO simultaneously changes all of the observable and unobservable neighborhood characteristics of program movers ($x\text{-}_{in}$ and $,_n$ from equation 1). The reduced-form parameters $"_1$ and $"_2$ in equation (3) thus reflect the combined effects of changing all of these neighborhood characteristics at once.

IV. DATA

Data for this analysis comes from four sources: baseline survey and follow-up address data, both collected by Abt Associates; administrative data on quarterly employment and earnings; and administrative data on public assistance (PA) receipt.

A. Baseline surveys

Applicants to the MTO program were required to complete a self-administered questionnaire designed by Abt Associates, which included questions about the householder's personal demographic characteristics as well as her educational attainment, current employment status (or job-search and training activities), and participation in social programs. The survey also includes questions about the age, educational attainment, employment status and relation to household head of others living in the household.

B. Post-program addresses

Abt is responsible for tracking the addresses of MTO families following their entry into the program. The first set of follow-up addresses provided to us by Abt are current as of July, 1997, and come from the administrative records that the local housing agencies maintain as part of their Section 8 voucher programs, as well as from other passive tracking sources such as change-of-address registries and credit bureaus.

A second set of Abt follow-up addresses are current as of July, 1998, and supplement the results of passive tracking sources through a brief follow-up survey of MTO families. Surveys were conducted on the phone for as many families as possible; those who could not be reached by telephone were interviewed in person. The survey included questions about the current composition of the household, the new addresses of people who were listed as members of the household on the baseline survey but no longer living with the householder, and the age, gender and relation to household head of new members of the household. The response rate to Abt's survey was 91 percent.[7]

C. Quarterly Employment and Earnings Data

The Maryland Department of Labor, Licensing and Regulation (DLLR) maintains complete quarterly employment and earnings histories for every person who has ever been employed in a private-sector job in Maryland that participates in the state's unemployment insurance (UI) system. These UI data enable us to construct employment and earnings histories that are less susceptible to misreporting problems such as recall error or self-presentation bias than survey data. The drawback is that these UI records do not capture public-sector jobs, informal work, or self employment, although formal work in a UI-covered job is an interesting outcome in its own right since this is an important indicator of economic success for the MTO population.

The DLLR used social security numbers, dates of birth, and first and last names to match our MTO participant lists with UI earnings records for the second quarter of 1985 through the first quarter of 1999.[8] The match rate that results from this process is of critical importance, since the estimated effect of MTO on employment and earnings in UI-covered jobs will be proportional to this match rate.

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[7] Personal communication with Debi Magri McInnis, Abt Associates.

[8] Prior to the first quarter of 1995, the UI system started each person's UI earnings history beginning with their second quarter of employment, and would thus omit the worker's first quarter in a private-sector job (starting in 1995:2, the system began to record each person's first quarter of work as well). This idiosyncracy of the UI reporting system is unlikely to be much of a problem in practice, since (as described in detail below) the large majority of MTO householders had already worked for pay at some point prior to enrolling in the program in late 1994 or early 1995. (Private communication with John Janak, Jacob France Center, University of Baltimore.)

For example, let W_E and W_C represent the proportion of householders who work in a UI-covered job during the post-program period in the experimental and control groups, respectively, and let M represent the match rate, which for simplicity is assumed to be equal across the two groups. As seen in equation (4), the estimated impact of MTO on UI-covered work equals the true effect multiplied by M.

(4) Estimated Impact = $(M \times W_E) - (M \times W_C) = M \times (W_E - W_C)$

Fortunately, there is reason to believe that the match rate is quite high – of those MTO householders who reported holding a job for pay at some point in her life, the DLLR found a UI earnings history in every case.

D. Public Assistance Receipt

The Maryland Department of Human Resources (DHR) maintains administrative records on receipt of public assistance (PA) cash benefits by residents of the state of Maryland, including the start and end date of every PA spell, and the monthly benefit amount for the family's most recent spell. In response to the Federal Personal Responsibility and Work Opportunities Reconciliation Act of 1996 (PRWORA), the state of Maryland now requires families receiving PA benefits to spend at least 20 hours per week in an acceptable work or training program beginning in the family's 24th month of participation in the Temporary Assistance for Needy Families (TANF) program. It is well-known that AFDC / TANF caseloads nationwide have decreased in recent years, and Maryland is no exception. If the recent reductions in welfare caseloads have changed the "welfare culture" in the MTO baseline neighborhoods, as many proponents of the PRWORA changes claim, our estimates may understate the effects of residential mobility on welfare receipt that would hold in times of high welfare caseloads.

The DHR used social security numbers to match our list of MTO participants with PA administrative records that are current as of August, 1998; in cases where no match was found, DHR searched again by the MTO participant's date of birth and completed the match using first and last name. Comparisons of self-reported welfare receipt on the baseline surveys with the state administrative data suggest that the match rate is on the order of 80 to 90 percent. A full 98 percent of MTO householders report that they have received AFDC benefits at some point during their lives; of this group, the DHR matched administrative AFDC / TANF records for 89 percent. When we focus on PA status at the time of program entry by comparing self-reported PA receipt on the baseline surveys with the DHR records, we find disagreement in the household's baseline PA status in 20 percent of cases. Almost all of the disagreements (106 of 122 families) consist of households who report that they receive PA benefits on the baseline surveys but are not recorded as active PA beneficiaries at that time in the DHR administrative database.[9]

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[9] The correspondence for the 609 householders who answered the PA question:

	DHR administrative data

V. EMPIRICAL RESULTS

In this section we begin by presenting the baseline characteristics and relocation outcomes of the MTO program population. We then examine the effects of MTO on employment and earnings, welfare receipt, and cohabitation.

A. Characteristics of the MTO Population

Table 1 presents information about MTO participants from the baseline surveys, and highlights the challenges that many families face in becoming economically self sufficient. Almost all of the MTO householders are African-American women, and all have children (by virtue of the program's design), many of whom will require child care if the mother goes to work. Only around half of MTO householders have either a high school diploma or GED, almost none had access to a car, and the large majority received AFDC at baseline. While the majority of householders report that they have held a job for pay at some point in their lives, only one-quarter were working at baseline. In his study of employers in several cities, Harry Holzer (1996) found that nearly 40 percent of firms filled their most recent non-college job through a referral from a current employee or acquaintance. Table 1 further supports the importance of informal social networks in the hiring process for less-skilled urban workers: Around two-thirds of all employed householders first heard about their current job from a neighbor, friend, or family member.

Despite the very low average earnings and employment rates reported in Table 1, most families did *not* enroll in MTO to gain access to better job opportunities. As shown in Table 2, around 80 percent of MTO applicants report that escaping gangs and drugs is the first or second most important reason for joining the program. This motivation is not surprising given that over half of the MTO applicants also report that at least one household member had been victimized by a crime during the past six months. While this victimization rate may be somewhat over- or under-stated due to telescoping and other reporting problems (Skogan, 1981), this figure is nevertheless substantially higher than the six-month victimization rate of six percent reported by residents of New York City public housing (Goering, Carnevale and Teodoro, 1996).

With random assignment, the characteristics of families should differ across the MTO treatment groups only by chance. That appears to be the case. Multivariate analysis of variance is used to test the null hypothesis that the full set of means presented in Tables 1 and 2 are equal across the three MTO groups (Johnson and Wichern, 1992). The relevant test statistic is consistent with the idea that the three groups are indistinguishable with respect to these observable characteristics [Wilks'

		Not on PA	On PA
Baseline survey data	Not on PA	106	382
	On PA	105	16

lambda=0.29, F(96,98)=0.87, p=.75].

B. Relocation Outcomes

Relative to the experimental group, a larger proportion of Section 8-only families relocated through the MTO program (73 versus 54 percent).[10] While relocation rates are higher among the Section 8-only group, the experimental families who relocate are more dispersed throughout Baltimore City and the larger metropolitan area, as seen by their July, 1997 addresses shown in Figure 1.

Table 3 provides more detailed information about the post-program neighborhoods of MTO families. By design, (nearly) all of the experimental relocators move to low-poverty census tracts with 1990 poverty rates below 10 percent,[11] and around 40 percent of those experimental families who relocate through MTO move outside of Baltimore City. In contrast to the experimental-group relocators, only around one in ten of the Section 8-only relocators voluntarily moved to census tracts with poverty rates under 10 percent. Table 3 also shows that the neighborhoods for the experimental group have proportionately more whites, high-income households, and college-educated adults than those for the Section 8-only group, and also have fewer high school dropouts or single-parent households and lower crime rates.

Finally, the MTO data can only help us identify the effects of neighborhoods and residential mobility on juvenile crime if mobility patterns among the experimental and Section 8-only groups are different from the controls. Table 3 shows that this is the case even through July, 1998, by which time all of the experimental families have completed their initial one-year leases and are free to relocate to higher- or lower-poverty neighborhoods as they wish. While some control group families moved to lower-poverty neighborhoods on their own, the 1998 addresses show that only 5 percent had moved to very low-poverty tracts (<10 percent) by this time.[12] In contrast, most of the experimental and

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[10] Of the Section 8-only families who did not relocate through MTO, almost all contacted the Baltimore housing office and requested a Section 8 subsidy, but then could not sign a lease before the subsidy offer expired. In contrast, only half of the experimental group non-relocators ran up against the Section 8 subsidy time limit. One-quarter of the experimental non-relocators did not successfully complete the mandatory CAN counseling program (and were thus not allowed to relocate), and the remaining quarter never contacted CAN after being assigned to the experimental group.

[11] As seen in Table 2, a small proportion of experimental relocators in Baltimore moved to census tracts with 1990 poverty rates slightly higher than 10 percent. HUD and Abt Associates quickly detected the pattern and worked with CAN to ensure that all experimental relocators chose neighborhoods that met the program poverty-level requirement.

[12] While the families in the control group received no mobility assistance under the MTO program, a HUD-funded Hope VI project demolished four public housing sites during our sample

Section 8-only relocators remain in neighborhoods that are quite similar to where they originally moved through the MTO program. Thus, unlike the EHAP experiments of the 1970's, the MTO experiment produces substantial changes in both mobility rates and the economic and social characteristics of participant's neighborhoods.

C. Earnings and Employment

Employment rates among MTO household heads in each of the three treatment groups increased dramatically during the post-program period (Table 5). Four quarters before families are randomized into a treatment group, only around one-fifth of householders work in a UI-covered job. Five years later (through the 17th quarter following random assignment), around half of all householders were employed. These changes are almost surely due in large part to the robust economy during the 1990's and the changes in Maryland welfare laws under PRWORA.

While employment rates rose for all MTO families during the post-program period, Table 5 shows little difference in these labor market outcomes across MTO treatment groups (Table 5). For example, only two of the 17 post-program differences between the experimental and control groups in quarterly employment rates are statistically significant at the 10 percent level, which is not much different from what we would expect from random chance. The findings are qualitatively similar when we focus on average quarterly earnings, or the natural logarithm of quarterly earnings. When we re-estimate these differences after regression-adjusting for random pre-program differences in baseline characteristics, including householder age, educational attainment, marital status, number of children, and employment during the eight quarters prior to random assignment, the results are almost identical (not shown).

While in principle MTO could have had an effect on job tenure or job transitions that is not reflected in quarterly employment or earnings rates[13], our empirical analysis provides no support for such changes. When we estimate logit models that examine the probability of a transition from not working to working from one quarter to the next, or the probability of getting a job or going from one

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period, including two located in the baseline census tracts (Lafayette Courts and Lexington Terrace.) Hence all families in these buildings, including around one-fifth of the families in the MTO control group, were forced to relocate either to other public housing buildings, or to private housing with Section 8 subsidies.

[13] For example, suppose that half of all control-group householders hold jobs during the entire post-program period, while the other half never works during this time. Also suppose also that relative to the baseline neighborhoods jobs may be more plentiful in low-poverty areas and informal childcare opportunities may be less plentiful. In this example, it might be the case that half of all experimental-group householders are working at any point in time, but householders alternate quarters between work and unemployment as they are forced to re-attend to their child care arrangements.

job to another, we find no systematic differences across MTO treatment groups (not shown).[14]

Another possibility is that MTO has some effect on the employment rates or earnings of particular sub-groups that are obscured by our analysis of the program population as a whole. For example, some analysts have hypothesized that social programs may have their greatest impacts on families who are "optimally constrained," defined as those whose labor market prospects are sufficiently strong such that they can take advantage of the opportunities offered by MTO, but not so strong that they will succeed even in the absence of the program. In order to explore this hypothesis, we created an index that measure the number of "constraints" that each householder faces by summing together indicator variables such as whether the householder is a high school dropout, whether the householder's mother was on welfare, whether the household contains has one child under six years of age (or two or more young children), and whether anyone in the home has a disability. We find no differences in program impacts when we stratify our analytic sample by the value of this index, which is robust to a number of different definitions for our "constraint" variable.

Finally, the household rather than household head might be the proper unit of analysis if MTO induces changes in the labor market involvement of teens and other secondary wage earners. Yet we find no evidence that households in the experimental or Section 8-only groups experienced higher

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[14] We estimate these logit models in a number of different ways, each of which produces quite similar findings. We construct a quarterly panel dataset for the 17 post-program quarters for which we have UI data that includes dummy indicators for whether the family was assigned to the experimental or Section 8-only groups, dummies indicating the quarter since random assignment, and interactions between the MTO treatment-group dummies and the quarter indicators. (The models also include controls for the householder's educational attainment, marital status, age, number of children, and variables indicating whether the householder was employed during each of the 12 quarters prior to random assignment). The trichotomous dependent variable indicates whether the householder's employment status was the same as for the previous quarter (no transition), whether the householder made a job-no job transition, or a no job - job transition. Unordered multinomial logit models suggest no difference in transition probabilities for any of the post-program quarters (standard errors are adjusted for the non-independence of observations in our panel dataset). Similar findings are obtained when we estimate dichotomous-outcome logit models where we condition on employment (non-employment) and examine the probability of transitioning to non-employment (employment). We also examine the probability of changing employers from one quarter to the next using the employer identification numbers that the Maryland UI system maintains as part of each worker's UI record. While our reliance on employer identification numbers will cause us to miss worker moves from one branch office of a firm to another, and to misclassify changes in firm ownership as changes in jobs, we find no differences across treatment groups in these transition probabilities.

employment rates or earnings relative to controls, as shown in Table 6.[15]

D. Welfare Receipt

In contrast to the null findings obtained from the UI quarterly employment and earnings data, we find that welfare receipt is lower among the experimental than Section 8-only or control groups (Table 7). Section 8-only householders appear to receive PA at rates above those for the experimental and control groups during the pre-program and early post-program periods, which raise questions about whether random assignment in MTO was done correctly by the program administrators. We believe that these pre-program differences in PA receipt are due to random chance in part because we do not observe any systematic differences across treatment groups in pre-program characteristics from the baseline surveys (Tables 1 and 2), and the pre-program differences in earnings and employment rates are not different from what we might expect due to chance (Tables 5 and 6). Moreover, there are no significant pre-program differences in *household-level* PA receipt (defined as PA receipt by anyone in the household), as shown in the last three columns of Table 7, and we observe no systematic differences in pre-program characteristics in the Boston MTO site (Katz, Kling, and Leibman, 1999).

In any case, we find that experimental group householders and households have lower rates of PA receipt than Section 8-only and control families throughout the post-program period, with three of the thirteen differences are significant at the 5 percent level (Table 7). On average, the likelihood that the experimental group is on welfare during a given quarter in the post-program period is around 9 percentage points lower than for the control group, a difference that is statistically significant at the 5 percent level. The Section 8-only group is around 4 percentage points less likely to be on welfare than controls, a difference that is not statistically significant.[16]

When we regression-adjust for random differences in PA receipt during the 8 quarters prior to random assignment, as well as for a number of other baseline characteristics such as householder educational attainment, age, marital status, and number of children , we produce even stronger evidence for a reduction in PA receipt among experimental-group families relative to controls (Table 8). We now also find some evidence that Section 8-only families may have lower rates of PA receipt than

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[15] We define the employment variable for households as equal to one if anyone in the household holds a job in a given quarter, where household members are defined as those who live with the household head at the time of the baseline survey. The household earnings variable is equal to the sum of earnings for every member of the household.

[16] These calculations come from stacking the quarter-by-quarter data on PA receipt into a panel, and estimating the average difference in PA receipt across MTO treatment groups using a probit model. Standard errors in these calculations are adjusted for the nonindependence of observations in this panel dataset.

controls through the first year and a half following random assignment, but these differences attenuate over time. For all of our comparisons, the standard errors are somewhat larger for the later periods because the sample of families for whom we have post-program data diminishes. This sample attrition should not affect the point estimates themselves, since the date on which families were drawn off of the MTO waiting list and randomly assigned into a treatment group is randomly determined.

E. Cohabitation

Another way that neighborhoods may affect economic self-sufficiency is by influencing the probability that the household head is married or cohabits with other adults. William Julius Wilson (1987) has argued that the supply of "marriageable men" in many high-poverty urban neighborhoods is depressed because of low employment and high incarceration rates. This hypothesis suggests that the MTO program may improve the marriage or cohabitation prospects for householders and thus improve the resources available to families (both financial and in terms of adult time). On the other hand, if the "market" for adult companions occurs at a geographic level larger than the neighborhood – for example, at the metropolitan-area level – then moving families from one part of the metropolitan area to another may have little effect on household composition. It is also possible that some changes in household composition may reduce rather than increase the ability of families to lift themselves from poverty. For example, relatives living in high-poverty neighborhoods in Baltimore may move in with MTO families to take advantage of the new housing and neighborhood opportunities provided by the program. While these additional household members could contribute time or money to the household, they could also in principle dilute the resources available per household member under some conditions. While the follow-up surveys conducted by Abt do not provide detailed information about the financial and other contributions of household members, learning more about changes in household composition across MTO treatment groups is of some interest in its own right.

We show in Table 9 that if anything, the primary effect of the MTO program is to *reduce* cohabitation rates among Section 8-only householders relative to experimentals or controls. The Abt follow-up surveys enable us to determine whether current members of the household were also in the home at baseline, and whether current members are adults or related to the household head. The top panel of Table 9 shows that the proportion of household heads who move in with an unrelated adult during the post-program period is almost double in the experimental and control groups than for Section 8-only group (7.9 and 7.0 percent versus 3.7 percent), though only the experimental / Section 8-only contrast is significant at the 10 percent level. We observe similar differences in the proportion of households who add any adult during the post-program period (either a cohabiting partner, adult relative, or adult child), as seen in the bottom two panels of Table 9.

Of particular interest is that we find very little difference in household composition between the experimental and control groups. These findings would seem to rule this out as a possible explanation for the differences in welfare receipt reported in the preceding section, though household heads may have some incentive to under-report cohabitation because of Maryland welfare rules [check].

VI. DISCUSSION

Concern about the concentration of poverty in America's inner cities has led in recent years to a shift in government policy from the operation of public housing developments to the provision of tenant-based subsidies such as housing vouchers. If voucher programs are voluntary, we can conclude that families who choose to make use of government housing vouchers are better off – the minimum standard that might be applied for judging the success of such programs. Yet policymakers frequently have higher aspirations for voucher programs as well, such as deconcentrating urban poverty and increasing economic and racial residential integration, and improving the material well-being of families by moving them into neighborhoods that are more conducive to economic success. Our findings suggest that providing public housing residents with housing vouchers has the potential to achieve both objectives, but the evidence is not always as clear as we would like.

Since families volunteer to participate in the MTO residential-mobility demonstration, it is not surprising that a substantial proportion of families make use of the Section 8 vouchers that are offered to them through the program's experimental and Section 8-only treatments. What is perhaps more surprising is that only around half of the families who are assigned to the experimental group take advantage of the offer to relocate. Around half of the experimental families who do not relocate never complete the 12 to 15 hours of counseling that are required by the local non-profit in order to receive a Section 8 voucher [check]. We also find some evidence that seems to suggest that some families do not relocate because of the realization that low-poverty areas may be relatively far from baseline neighborhoods (and thus family, friends and jobs), since families with more relatives and friends in their baseline communities are less likely to relocate. Other families may have had trouble finding private-market apartments that are located in low-poverty areas in Baltimore, especially apartments that are large enough to accommodate their children. Even one-quarter of the families who are assigned to the Section 8-only group, whose relocation decisions are not constrained by the MTO program rules, do not take advantage of the opportunity to move to a private-market apartment. Presumably many of those in the Section 8-only group who do not relocate had trouble finding an acceptable private-market apartment in even neighborhoods with moderate poverty levels where the landlord would accept payment in the form of a Section 8 voucher.

In any case, our descriptions of the relocation outcomes of MTO families show that the offer of vouchers changes the rate at which public housing families move and the conditions of the neighborhoods in which families live. The differences in mobility rates across MTO treatment groups is not surprising, since families in the control group who wish to move must either make their way to the top of the Housing Authority of Baltimore City's Section 8 wait list, or must forego the housing subsidy they receive for their public housing unit and pay for a private-market apartment out-of-pocket. The fact that families in the experimental group who relocate move to very low-poverty areas is simply the result of the MTO program's design. What is more interesting, however, is the contrast in relocation neighborhoods between the experimental and Section 8-only movers. Relatively few of the Section 8-only movers voluntarily relocate to very low-poverty census tracts, and (as seen in Figure 1) most

choose to stay quite close to the baseline neighborhoods. These findings suggest that if one objective of social policy is to integrate very low-income families into very low-poverty neighborhoods (or into the suburbs more generally), programs must include either additional subsidies or penalties to induce families to make such moves.

Our findings for the effects of MTO on economic self-sufficiency present us with something of a puzzle: While we find no differences in quarterly employment rates or earnings in private-sector jobs across MTO treatment groups, the experimental group seems to experience a reduction in welfare receipt relative to controls. The key question is whether the reduction in welfare receipt signals that MTO improves the material well-being of families in ways that are not captured by the UI earnings data, or instead if experimental-group families exit welfare for some reason (such as stigma) despite the negative consequences for their standard of living.

One explanation for our findings is that MTO experimental-group families experience an increase in income that is not reported to the state's UI system. This explanation has substantial face validity in part because Kathryn Edin and Laura Lein (1997) have documented that almost all welfare recipients supplement AFDC benefits with other sources of income, primarily from under-the-table earnings or income from friends, relatives, boyfriends and ex-husbands. In fact, these unreported sources of income typically accounted for around half of these families' total income. Opportunities to earn unreported income may be greater in lower-poverty neighborhoods because of improved access to employers (some of whom may be willing to hire people off-the-books), or because more affluent neighborhood residents have greater demand for off-the-books services such as daycare or housekeeping.

A related explanation is that experimental-group families experience an increase in hourly wages in the formal labor market, and respond to this change by reducing their labor supply thus leaving total monthly earnings unchanged. While this labor-supply response to an increase in hourly wages is not consistent with evidence for how low-income women respond to changes in the Earned Income Tax Credit program (Meyer and Rosenbaum, 1998), in principle the responses of the self-selected MTO householders may be different from the average low-income woman. For example, MTO householders appear to be unusually concerned about the involvement of their adolescent children in criminal activity (Ludwig, Duncan and Hirschfield, 1999), and as a result may place an unusually high premium on spending time at home. Under this explanation, families exit welfare (thereby conserving welfare eligibility under the new TANF time limits) because of an increase in total resources – income plus parent time. Unfortunately we have no way to test this possibility directly, since the Maryland UI records do not provide information on hourly wages.

Finally, it is possible that experimental-group relocators succumb to social pressures against welfare receipt in their new neighborhoods, and choose to exit welfare even if this means a substantial reduction in their family's income. Yet even if this is true, relocators must still be better off on net (even after giving up their welfare benefits) because they always have the option of moving to a different

neighborhood with different social pressures once their initial one-year lease expires. As it turns out, most experimental and Section 8-only relocators choose to stay in their original destinations or similar neighborhoods after their initial leases are up.

In our view, an increase in earnings that are not captured by the UI data is the most plausible explanation. This judgement is based in part on qualitative data gathered by Helen Ladd and Jens Ludwig, who conducted follow-up surveys of Baltimore MTO families during 1997 and 1998. Ladd and Ludwig surveyed 121 of the 143 experimental-group families (85 percent) who had been randomized through April 1995 and had successfully relocated through the MTO program, and 83 of the 141 Section 8-only families (59 percent) who had been randomly assigned by our cutoff date. Since the interviewed householders are a selected subset of all movers, the findings from these surveys are only suggestive. But the fact that around two-thirds of experimental-group relocators report that job and training opportunities are better in their new neighborhood (Table 10) is at least consistent with the explanation that families experience increases in earnings that are not captured by the state UI records.[17] The survey results presented in Table 10 also suggest another reason why the effects of the experimental treatment on employment and welfare receipt are not larger – only one third of program movers report that child care opportunities are better after the move. In fact, nearly 40 percent of those who report that job or educational opportunities are *better* in their new neighborhood also report that child care opportunities are *worse*.[18] The proportion of Section 8-only relocators who report that job and training opportunities are better in their new neighborhoods is smaller than what is observed for the experimental movers, which is consistent with our findings that the experimental group experiences a larger reduction in welfare participation.

The long-term effects of the MTO experimental treatment on economic self-sufficiency may be different from those that we estimate in this paper. On the one hand, the social capital available to families in their new neighborhoods is likely to increase over time, which in turn should improve both job contacts and opportunities for informal child care. Further, if some experimental and Section 8-only households take advantage of new educational opportunities by enrolling in school or training programs, this will reduce employment rates in the short run but increase long-run employment and earnings. On the other hand, some experimental-group families may decide to move back into their old neighborhoods (though we have seen little evidence of this through the first few years of the program), which would depress any long-term differences across treatment groups.

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[17] Householders are asked "Do you think the job opportunities for you are better in your old or new neighborhood?", and "Do you think the opportunities for you to go to school or get training are better in your old or new neighborhood?"

[18] Householders are asked "Do you think the opportunities for you to provide day care or find someone to watch for your child (or children) are better in your new neighborhood or in your old neighborhood?"

Whether large-scale housing-mobility programs on net improve social welfare is a question that cannot be answered by the MTO experiment. While we find some evidence that families who volunteer for MTO and are given the opportunity to move to very low-poverty neighborhoods experience reductions in welfare receipt, these findings may not generalize to other low-income populations. The fact that Section 8-only families do not appear to experience sustained differences in welfare receipt suggests that mobility programs may need to require families to move to very low-poverty areas if these policies are to change economic outcomes. But most importantly, the effects of large-scale mobility programs may be different if families cluster together in new neighborhoods, which was intentionally prohibited among experimental families by the non-profit group that provided relocation counseling. And our MTO analysis tells us nothing about the effects of either small- or large-scale mobility programs on the well-being of families in host neighborhoods.

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Table 1
Baseline Characteristics of MTO Householders from Baseline Survey Data

	Total	Experimental	Section 8-Only	Control
Families (N)	638	252	188	198
Householder characteristics:				
African-American (%)	97.4	96.8	97.2	98.4
Female householder (%)	94.7	96.0	92.0	95.5
Householder age	35.1	35.8	34.3	34.8
Number of children	2.62	2.57	2.75	2.55
Has h.s. degree	41.7	44.1	45.8	34.8
Has G.E.D.	14.9	15.0	13.0	16.6
Married	3.5	3.3	4.0	3.3
Has driver's license	20.2	17.5	27.4	16.9
Has car that runs	4.1	4.8	4.3	3.0
Householder Earnings/Work:				
Household income ($'s)	6,876	6,894	6,679	6,750
AFDC at baseline	80.3	79.3	81.6	80.4
AFDC ever	97.6	97.2	97.2	98.4
School or training at baseline	15.8	15.1	16.5	16.2
Has never worked	13.2	14.8	9.9	14.2
Worked all 4 quarters prior to enrolling in MTO	11.4	10.7	9.4	14.1
Work full/part-time baseline*	23.0	22.3	19.3	27.2
Tenure current job (weeks)	106.2	95.6	95.5	125.2
Hours worked per week	31.2	31.0	29.2	32.8
Wages per hour ($'s)	5.98	5.59	6.68	5.95
Commuting (employed householders):				
Commute under 15 minutes	21.9	21.0	22.2	22.6
60 minutes or more	6.0	6.5	2.8	7.5
Commute by public transp	54.7	51.7	62.9	52.8
Own car	4.1	5.0	0	5.7
Walk	33.8	33.3	31.4	35.8
Carpool	2.0	1.7	2.9	1.9
How householder heard about current job:				
Friend, neighbor, family	60.7	57.6	64.7	61.5
Want ad	0.7	1.7	0	0
Employment agency	34.5	33.9	32.4	36.5
Welfare office	2.1	5.1	2.9	0
Other	2.1	1.7	2.9	1.9

NOTES:* Includes respondents who work part-time and also attend school or training programs (between 1.2 and 2.5 percent of all respondents, or about one-tenth of the group that is working at the time of the

baseline survey).

Table 2
Motivations for Enrolling in MTO Program

	Total	Experimental	Section 8-Only	Control
Criminal Victimization				
During last 6 months, someone in HH:				
Had valuable snatched	23.3	22.6	25.6	22.0
Beaten/assaulted	27.7	31.7	24.6	25.7
Stabbed/shot	11.9	12.8	10.1	12.6
Break-in to home	25.9	27.3	27.9	22.0
Any of above	51.7	55.3	51.7	47.1
Primary reason for wanting to move:				
Better schools	11.7	9.8	14.4	11.5
To be near job	0.5	0	1.1	0.5
Better transportation	0	0	0	0
To get a job	1.0	1.2	0.6	1.0
Avoid gangs, drugs	53.5	53.3	52.2	55.0
Better apartment	25.1	26.4	23.9	24.6
Other	4.7	4.5	3.9	5.8
Second most important reason for move:				
Better schools	30.3	30.1	33.3	27.7
To be near job	0.6	0.4	1.1	0.5
Better transportation	0.3	0.4	0	0.5
To get a job	4.7	6.1	3.3	4.2
Avoid gangs, drugs	27.1	27.2	25.0	28.8
Better apartment	28.0	25.2	30.0	29.8
Other	4.7	6.1	3.3	4.2

NOTES:* Includes respondents who work part-time and also attend school or training programs (betwee 1.2 and 2.5 percent of all respondents, or about one-tenth of the group that is working at the time of the baseline survey). ** Defined as purse-snatching, threatened with gun or knife, beaten/assaulted, stabbed/shot, and break in to home.

Table 3 Relocation Outcomes for MTO Families

	Baseline (all families)	Experimental		Section 8-only		Control	
	1994-1996	1997	1998	1997	1998	1997	1998
Distribution of MTO Households							
Jurisdiction :							
Baltimore City	100.0	77.1	79.4	89.9	86.7	99.5	98.0
Anne Arundel County	0.0	0.8	2.0	0.0	0.5	0.0	0.0
Baltimore County	0.0	13.0	10.7	5.3	8.0	0.0	1.0
Harford County	0.0	0.4	0.4	0.0	0.0	0.0	0.0
Howard County	0.0	7.1	5.9	2.7	2.7	0.0	0.5
Montgomery County	0.0	0.4	0.4	0.0	0.0	0.0	0.0
Other	0.0	1.2	1.2	2.1	2.1	0.5	0.5
% Census Tract Poor:							
0 - 9.9	0.0	49.4	43.0	8.7	12.5	0.0	4.5
10 -19.9	0.0	4.8	8.4	14.7	21.2	0.0	7.6
20- 29.9	0.2	0.0	7.6	10.3	15.8	0.0	3.0
30- 39.9	0.3	0.4	4.0	12.5	13.0	0.0	6.6
40- 49.9	2.0	1.6	6.4	9.8	7.1	2.0	6.6
50-59.9	4.4	1.2	4.0	6.5	4.9	5.6	4.5
60-69.9	52.5	22.7	18.7	26.6	19.6	49.0	43.4
70-79.9	20.4	9.6	4.0	7.1	3.8	23.2	14.6
80 plus	20.1	10.4	4.0	3.8	2.2	20.2	9.1
Mean Census-Tract Characteristics							
Index Crimes [a] (per 1,000 pop.)	194.2	145.9	140.4	159.4	145.4	196.7	173.2
% White	8.2	33.4	34.7	20.9	28.1	8.5	14.1
% Under Age 20	40.1	33.1	30.6	34.3	31.7	40.1	36.5
% Age 20-34	27.7	27.9	27.6	27.3	27.7	27.9	27.3
% Over 35 Years of Age	32.3	39.1	41.7	38.4	40.6	32.0	36.2
% Adults w/out HS Degree	54.2	38.4	37.8	47.7	44.6	54.3	51.0
% Adults w/ College Degree	6.5	15.3	15.5	9.2	10.6	6.7	7.6
% HHs headed by female	80.6	52.0	46.2	59.5	51.3	80.9	68.6
% HH w/ inc >=$50,000	4.7	16.5	16.8	8.7	10.7	4.5	7.6

NOTES: Neighborhood characteristics are calculated using 1990 Census data. a. The FBI's Uniform Crime Report index crimes are homicide,

forcible rape, robbery, assault, breaking and entering, larceny-theft, motor vehicle theft, and arson (Maryland State Police, 1997).

Table 4
Predictors of Residential Relocation through MTO Program

	Dependent variable: Residential relocation through MTO program (=1 if yes, =0 else)	
	Experimental group	Section 8-only group
Intercept	0.78 (0.19)**	1.07 (0.19)**
Household char's:		
Householder age (years)	0.005 (0.004)	0.01 (0.004)**
Number of children	-0.05 (0.02)**	0.03 (0.02)
High school degree	0.03 (0.07)	-0.05 (0.08)
GED	0.01 (0.10)	-0.20 (0.10)*
Has driver's license	0.03 (0.09)	-0.04 (0.08)
Work at baseline:		
Full / part time[a]	-0.03 (0.08)	-0.08 (0.08)
Small jobs	0.07 (0.12)	0.25 (0.12)**
On AFDC	0.19 (0.09)**	0.05 (0.10)
"Social capital" in baseline neighborhoods:		
Many friends	-0.17 (0.13)	0.03 (0.13)
Some friends	-0.03 (0.07)	0.04 (0.07)
Many family	-0.26 (0.20)	-0.21 (0.16)
Some family	-0.09 (0.08)	-0.08 (0.08)
N	227	170
R-squared	0.10	0.11

NOTES: Standard errors in parentheses. ** = Statistically significant at 5 percent level. * = Statistically significant at 10 percent level. a = Householder defined as working full or part time at baseline if the householder either reports working on the baseline survey or has positive earnings during the baseline quarter in the UI earnings database.

Table 5
Quarterly Employment and Earnings for MTO Householders

	Percent householders employed			Quarterly earnings for employed householders (in thousands)		
	Exp	S8-Only	Control	Exp	S8-Only	Control
Quarters Since Random Assignment:						
-4	0.19 (0.02)**	0.21 (0.03)*	0.29 (0.03)	2.13 (0.22)+	1.63 (0.21)	2.11 (0.23)
-3	0.23 (0.03)	0.20 (0.03)	0.26 (0.03)	1.95 (0.20)	1.97 (0.25)	1.81 (0.23)
-2	0.23 (0.03)+	0.30 (0.03)	0.29 (0.03)	2.14 (0.19)	1.69 (0.20)	2.10 (0.24)
-1	0.27 (0.03)	0.29 (0.03)	0.29 (0.03)	2.18 (0.20)	1.91 (0.22)	2.03 (0.26)
0	0.25 (0.03)	0.27 (0.03)	0.30 (0.03)	2.42 (0.17)+	1.94 (0.20)	2.21 (0.27)
1	0.28 (0.03)*	0.29 (0.03)	0.35 (0.03)	2.16 (0.18)	1.94 (0.20)	2.23 (0.21)
2	0.34 (0.03)	0.34 (0.03)	0.38 (0.03)	2.08 (0.15)	2.01 (0.21)	2.04 (0.19)
3	0.38 (0.03)	0.36 (0.03)	0.40 (0.03)	2.11 (0.15)	2.18 (0.20)	2.28 (0.24)
4	0.40 (0.03)	0.38 (0.04)	0.41 (0.04)	2.15 (0.15)	2.16 (0.21)	2.13 (0.18)
5	0.39 (0.03)	0.37 (0.04)	0.45 (0.04)	2.23 (0.15)	2.29 (0.19)	2.22 (0.17)
6	0.44 (0.03)	0.40 (0.04)*	0.49 (0.04)	2.33 (0.14)	2.32 (0.19)	2.21 (0.16)
7	0.44 (0.03)	0.41 (0.04)	0.46 (0.04)	2.48 (0.17)	2.59 (0.35)	2.27 (0.20)
8	0.42 (0.03)	0.42 (0.04)	0.49 (0.04)	2.48 (0.17)	2.61 (0.23)	2.49 (0.19)
9	0.42 (0.03)*	0.42 (0.04)	0.50 (0.04)	2.51 (0.16)	3.09 (0.41)	2.60 (0.18)
10	0.45 (0.03)	0.45 (0.04)	0.50 (0.04)	2.64 (0.18)	2.53 (0.22)	2.62 (0.17)
11	0.43 (0.03)	0.49 (0.04)	0.47 (0.04)	2.71 (0.16)	2.44 (0.21)	2.85 (0.19)
12	0.44 (0.03)	0.46 (0.04)	0.47 (0.04)	2.66 (0.17)	2.57 (0.19)	2.57 (0.20)
13	0.48 (0.03)	0.46 (0.06)	0.49 (0.04)	2.42 (0.17)	2.51 (0.29)	2.34 (0.21)
14	0.45 (0.03)	0.40 (0.05)	0.48 (0.04)	2.61 (0.19)	2.91 (0.34)*	2.24 (0.21)
15	0.51 (0.03)+	0.40 (0.05)*	0.52 (0.04)	2.55 (0.17)	3.03 (0.32)*	2.38 (0.21)
16	0.51 (0.04)	0.49 (0.06)	0.51 (0.05)	2.66 (0.19)	3.20 (0.39)	3.01 (0.32)
17	0.53 (0.04)	0.46 (0.07)	0.48 (0.05)	2.47 (0.19)+	3.21 (0.39)	2.49 (0.28)

NOTES: * = Difference with control group significant at 10 percent. ** = Difference with control group significant at 5 percent. + = Difference between experimental and Section 8-only group significant at 10 percent. ++ = Difference between experimental and Section 8-only group significant at 5 percent.

Table 6
Quarterly Employment and Earnings for MTO Households

	Percent households w/ employed member			Quarterly earnings for employed householders		
	Exp	S8-Only	Control	Exp	S8-Only	Control
Quarters Since Random Assignment:						
-4	0.21 (0.03)**	0.24 (0.03)	0.31 (0.03)	2.12 (0.21)	1.68 (0.20)*	2.34 (0.28)
-3	0.25 (0.03)	0.22 (0.03)	0.28 (0.03)	1.98 (0.20)	1.98 (0.24)	2.21 (0.32)
-2	0.23 (0.03)**,++	0.34 (0.03)	0.32 (0.03)	2.35 (0.24)++	1.69 (0.19)*	2.38 (0.31)
-1	0.29 (0.03)	0.29 (0.03)	0.31 (0.03)	2.21 (0.22)	1.99 (0.22)	2.34 (0.31)
0	0.27 (0.03)	0.30 (0.03)	0.33 (0.03)	2.41 (0.17)+	1.91 (0.19)	2.47 (0.30)
1	0.30 (0.03)*	0.34 (0.03)	0.38 (0.03)	2.22 (0.20)	1.91 (0.20)*	2.49 (0.25)
2	0.40 (0.03)	0.38 (0.04)	0.42 (0.04)	1.97 (0.16)	1.97 (0.20)	2.20 (0.23)
3	0.44 (0.03)	0.38 (0.04)	0.45 (0.04)	2.08 (0.15)	2.27 (0.21)	2.36 (0.24)
4	0.44 (0.03)	0.40 (0.04)	0.45 (0.04)	2.17 (0.14)	2.20 (0.22)	2.37 (0.23)
5	0.43 (0.03)	0.41 (0.04)*	0.49 (0.04)	2.30 (0.15)	2.37 (0.20)	2.41 (0.20)
6	0.50 (0.03)	0.44 (0.04)*	0.54 (0.04)	2.31 (0.17)	2.35 (0.22)	2.48 (0.21)
7	0.49 (0.03)	0.44 (0.04)	0.52 (0.04)	2.52 (0.19)	2.69 (0.34)	2.39 (0.21)
8	0.47 (0.03)	0.45 (0.04)*	0.54 (0.04)	2.51 (0.18)	2.72 (0.25)	2.63 (0.21)
9	0.46 (0.03)	0.43 (0.04)*	0.53 (0.04)	2.77 (0.24)	3.19 (0.41)	2.73 (0.20)
10	0.50 (0.03)	0.48 (0.04)	0.55 (0.04)	2.76 (0.19)	2.68 (0.25)	2.75 (0.20)
11	0.48 (0.03)	0.51 (0.04)	0.54 (0.04)	2.81 (0.17)	2.69 (0.25)	2.77 (0.19)
12	0.49 (0.03)	0.48 (0.04)	0.52 (0.04)	2.76 (0.17)	2.74 (0.21)	3.45 (0.81)
13	0.51 (0.03)	0.49 (0.06)	0.53 (0.04)	2.63 (0.18)	2.76 (0.27)	2.39 (0.23)
14	0.45 (0.03)	0.40 (0.05)	0.48 (0.04)	3.11 (0.22)	3.23 (0.36)	2.64 (0.24)
15	0.51 (0.03)+	0.40 (0.05)*	0.52 (0.04)	3.25 (0.24)	3.40 (0.33)*	2.70 (0.25)
16	0.51 (0.04)	0.49 (0.06)	0.51 (0.05)	3.23 (0.27)	3.42 (0.39)	3.39 (0.36)
17	0.53 (0.04)	0.46 (0.07)	0.48 (0.05)	2.85 (0.23)	3.41 (0.43)	2.80 (0.31)

NOTES: * = Difference with control group significant at 10 percent. ** = Difference with control group significant at 5 percent. + = Difference between experimental and Section 8-only group significant at 10 percent. ++ = Difference between experimental and Section 8-only group significant at 5 percent.

Table 7
Quarterly Public-Assistance Receipt by MTO Householders and Households

	Percent householders receiving PA			Percent households receiving PA		
	Exp	S8-Only	Control	Exp	S8-Only	Control
Quarters Since Random Assignment:						
-4	0.43 (0.03)	0.49 (0.04)*	0.40 (0.04)	0.65 (0.03)	0.67 (0.04)	0.63 (0.03)
-3	0.44 (0.03)	0.50 (0.04)**	0.40 (0.04)	0.66 (0.03)	0.66 (0.04)	0.62 (0.04)
-2	0.44 (0.03)++	0.54 (0.04)**	0.40 (0.04)	0.65 (0.03)	0.69 (0.03)	0.63 (0.03)
-1	0.45 (0.03)++	0.57 (0.04)**	0.43 (0.04)	0.64 (0.03)	0.72 (0.03)	0.64 (0.03)
0	0.45 (0.03)++	0.56 (0.04)**	0.45 (0.04)	0.64 (0.03)	0.71 (0.03)	0.64 (0.03)
1	0.45 (0.03)++	0.56 (0.04)**	0.46 (0.04)	0.64 (0.03)	0.70 (0.03)	0.65 (0.03)
2	0.46 (0.03)+	0.54 (0.04)	0.48 (0.04)	0.63 (0.03)	0.66 (0.04)	0.64 (0.03)
3	0.44 (0.03)	0.51 (0.04)	0.48 (0.04)	0.62 (0.03)	0.62 (0.04)	0.63 (0.03)
4	0.41 (0.03)	0.48 (0.04)	0.47 (0.04)	0.56 (0.03)	0.56 (0.04)	0.60 (0.04)
5	0.37 (0.03)**,+	0.46 (0.04)	0.48 (0.04)	0.51 (0.03)*	0.52 (0.04)	0.60 (0.04)
6	0.38 (0.03)	0.45 (0.04)	0.43 (0.04)	0.49 (0.03)	0.51 (0.04)	0.55 (0.04)
7	0.38 (0.03)+	0.47 (0.04)	0.42 (0.04)	0.49 (0.03)	0.55 (0.04)	0.55 (0.04)
8	0.37 (0.03)++	0.48 (0.04)	0.39 (0.04)	0.46 (0.03)	0.53 (0.04)	0.50 (0.04)
9	0.34 (0.03)*,++	0.51 (0.06)	0.44 (0.04)	0.41 (0.03)**,++	0.60 (0.05)	0.52 (0.04)
10	0.33 (0.03)**,++	0.51 (0.06)	0.44 (0.04)	0.39 (0.03)**,++	0.56 (0.06)	0.52 (0.04)
11	0.37 (0.03)++	0.50 (0.06)	0.45 (0.04)	0.40 (0.03)++	0.54 (0.05)	0.48 (0.04)
12	0.38 (0.04)	0.48 (0.06)	0.46 (0.05)	0.41 (0.04)	0.48 (0.06)	0.48 (0.05)
13	0.33 (0.04)**	0.43 (0.07)	0.47 (0.05)	0.34 (0.04)*	0.43 (0.07)	0.47 (0.05)

NOTES: * = Difference with control group significant at 10 percent. ** = Difference with control group significant at 5 percent. + = Difference between experimental and Section 8-only group significant at 10 percent. ++ = Difference between experimental and Section 8-only group significant at 5 percent.

Table 8

Differences in Quarterly Public-Assistance Receipt by MTO Householders and Households

	Percent householders receiving PA		Percent households receiving PA	
	Exp vs Control	S8-Only vs Control	Exp vs Control	S8-Only vs Control
Quarters Since Random Assignment:				
0	-0.02 (0.01)	-0.01 (0.02)	-0.01 (0.01)	-0.01 (0.02)
1	-0.03 (0.02)*	-0.02 (0.02)	-0.02 (0.02)	-0.01 (0.03)
2	-0.06 (0.03)**	-0.05 (0.03)*	-0.03 (0.02)	-0.02 (0.03)
3	-0.08 (0.03)**	-0.06 (0.03)*	-0.04 (0.03)	-0.04 (0.03)
4	-0.08 (0.04)**	-0.05 (0.04)	-0.08 (0.04)**	-0.08 (0.04)*
5	-0.11 (0.04)**	-0.09 (0.04)**	-0.10 (0.04)**	-0.11 (0.04)**
6	-0.07 (0.04)	-0.03 (0.05)	-0.08 (0.04)*	-0.05 (0.05)
7	-0.06 (0.05)	0.00 (0.06)	-0.08 (0.05)	-0.01 (0.05)
8	-0.04 (0.05)	0.05 (0.06)	-0.05 (0.05)	0.04 (0.06)
9	-0.10 (0.05)*	0.01 (0.07)	-0.09 (0.05)*	0.04 (0.06)
10	-0.11 (0.05)*	0.03 (0.07)	-0.11 (0.05)**	0.02 (0.07)
11	-0.08 (0.06)	0.02 (0.07)	-0.08 (0.06)	0.04 (0.07)
12	-0.07 (0.06)	-0.02 (0.08)	-0.05 (0.06)	-0.01 (0.07)
13	-0.09 (0.07)	-0.05 (0.09)	-0.08 (0.07)	-0.04 (0.08)

NOTES: * = Difference significant at 10 percent. ** = Difference significant at 5 percent.

Table 9
Cohabitation and Household Composition for MTO Households, Pre- and Post-Program

	Exp (N=252) (%)	S8-Only (N=188) (%)	Control (N=198) (%)
Cohabit w/ Non-related Adult of Opposite Sex			
Pre-Program	2.0 (0.9)	3.2 (1.3)	1.5 (0.9)
Post-Program	9.9 (1.9)	6.9 (1.9)	8.1 (1.9)
Pre- & Post-Program	2.0 (0.9)	3.2 (1.3)	1.0 (0.7)
Pre-Program Only	0.0 (0.0)	0.0 (0.0)	0.5 (0.5)
Post-Program Only	7.9 (1.7)+	3.7 (1.4)	7.0 (1.8)
Cohabit or Other Adult in Home (not including adult children)			
Pre-Program	2.8 (1.0)	4.3 (1.5)	2.0 (1.0)
Post-Program	9.9 (1.9)	8.0 (2.0)	8.1 (1.9)
Pre- & Post-Program	2.8 (1.0)	4.3 (1.5)**	1.0 (0.7)
Pre-Program Only	0.0 (0.0)*	0.0 (0.0)*	1.0 (0.7)
Post-Program Only	7.1 (1.6)	3.7 (1.4)	7.1 (1.8)
Cohabit or Any Adult in Home (including adult children)			
Pre-Program	11.1 (2.0)	12.8 (2.4)	11.1 (2.2)
Post-Program	32.9 (3.0)+	24.5 (3.1)	28.3 (3.2)
Pre- & Post-Program	9.1 (1.8)	10.1 (2.2)	9.6 (2.1)
Pre-Program Only	2.0 (0.9)	2.7 (1.2)	1.5 (0.9)
Post-Program Only	23.8 (2.7)++	14.4 (2.6)	18.7 (2.8)

NOTES: Standard errors in parentheses. ** = Difference in comparison to control group is statistically significant at 5 percent level. * = Difference in comparison to control group is statistically significant at the 10 percent level. ++ = Difference in comparison to the Section 8-only group is statistically significant at the 5 percent level. + = Difference in comparison to the Section 8-only group is statistically significant at the 10 percent level.

Table 10
Self-Reported Changes in Economic Opportunities

	Experimental group relocators (N=120)	**Comparison group relocators** (N=57)
JOB opportunities better in new or old nghbhd?		
Old	15.8	5.3
New	65.8	52.6
Same	12.3	24.6
Don't Know	6.1	17.5
Opportunities for householder to go to SCHOOL or get TRAINING better in new or old nghbhd?		
Old	24.2	15.8
New	62.5	42.1
Same	9.2	21.1
Don't Know	4.2	17.5
Opportunities for CHILD CARE better in new or old nghbhd?	47.8	15.8
Old	35.4	45.6
New	5.3	15.8
Same	11.5	22.8
Don't Know		
% Who report:		
Job opportunities better in new nghbhd, AND Child care opportunities better in old nghbhd	24.8	5.3
Job opportunities better in new nghbhd, AND Child care opportunities better/same in old nghbhd	30.1	8.8
% Who report:		
School/training opportunities better in new nghbhd, AND Child care opportunities better in old nghbhd	25.9	5.3
School/training opportunities better in new nghbhd, AND Child care opportunities better or same in old nghbhd	28.6	8.8

NOTES: Results taken from follow-up surveys of relocators. Response rates for follow-up surveys reported in text.